Regularized Synthetic Control Methods:

Advancing Causal Inference in Time Series Econometrics and Observational Studies

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Abstract

The Synthetic Control (SC) method is a widely used tool for measuring causal treatment effects in observational trials. Typically, the counterfactual of the single treated unit is synthesized using a weighted average of the remaining units in the post-treatment phase. These weights are computed in a data-driven manner and aim to minimize the distance between the treated unit and its counterfactual in the pre-treatment phase. To avoid overfitting the training data and to ensure external validity of the results, the method's developers (Abadie, Diamond, and Hainmueller (ADH)) incorporated the constraint that each weight must be weakly positive, and all weights must up sum to one. Building on the work of [Doudchenko and Imbens, 2016], we propose a generalization that allows for the inclusion of a constant term and negative weights. However, we develop the Regularized Synthetic Controls (REGSC) estimator, an alternative regularization approach that shrinks individual coefficients towards zero and the sum of coefficients towards one. Besides the crucial advantage of a closed form expression, Monte Carlo studies confirm that this regularization method dominates other estimators in data-generating processes with factor structure as was proposed by the inventors of SC. Next, we extend the approach to dynamic contexts and propose a regularized Autoregressive Distributed Lag (ARDL) model for optimal estimation of the counterfactual in time series settings. Again, simulations confirm the new method's potential to enhance the accuracy and robustness of causal effect estimation in time series econometrics and observational studies.

Keywords: *Synthetic Control; Observational Studies; Causal Inference; Regularization, Autoregressive Distributed Lag Models*

List of Acronyms

ADH Abadie, Diamond, and Hainmueller

ARDL Autoregressive Distributed Lag

CV Cross Validation

GDP Gross Domestic Product

DGP Data Generating Process

iid independent and identically distributed

MSE Mean Squared Error

MSFE Mean Squared Forecast Error

MSPE Mean Squared Prediction Error

MZ Mincer-Zarnowitz

OLS Ordinary Least Squares

PC Principal Components

REGSC Regularized Synthetic Controls

RMSFE Root Mean Squared Forecast Error

RSS Residual Sum of Squares

SC Synthetic Control

USA United States of America

VAR Vector Autoregression

VARSC Vector Autoregressive Synthetic Control

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1. Introduction

This is work in progress. Goal: 1 page

SC method is combined with Vector Autoregression (VAR). Method was introduced by ADH. Paper strcuture could be similar to the one by [Doudchenko and Imbens, 2016]

2. Literature Review 2-3 pages

Literature has been clustered. This is work in progress

2.1. Synthetic Control

The SC method was developed by Alberto Abadie and colleagues in a series of influential papers ([Abadie and Gardeazabal, 2003], [Abadie et al., 2010], [Abadie et al., 2015]). The method is designed to estimate the causal effect of a treatment in a setting with a single treatment unit and a number of potential control units. Pre- and post-treatment data are observed for the treatment and control units for the outcome of interest as well as for a set of covariates. The SC-procedure combines aspects of the matching and difference-in-difference literature and can therefore be interpreted as a relative of the causal inference literature introduced by [Rubin, 1974]. Similar to many other microeconometric methods, the objective is to distinguish causation from correlation and to assess the magnitude and significance of treatments in observational case studies.

In their canonical 2003 article, Abadie and Gardeazabal evaluate the causal economic effects of conflict using terrorist conflicts in the Basque Country as a comparative case study. In their specific application example, they find that terrorist conflicts caused the per capita Gross Domestic Product (GDP) of the treatment unit (Basque Country) to decline by about 10% relative to the synthesized control unit.

some more words on other findis and things they did The next appropriate setting for an application of the SC method was the introduction of a large-scale tobacco control program implemented in the state of California in the United States of America (USA) in 1988.

2.2. Overview

[Abadie, 2021] read.

[Athey and Imbens, 2016] read.

2.3. Application

[Born et al., 2019] read.

[Cho, 2020] read.

[Cunningham, 2021] read.

[Funke et al., 2020] read.

2.4. Methodological Background

[Hainmueller et al., 2011] read.

[Abadie and Imbens, 2006] not read.

[Abadie and Imbens, 2002] not read.

[Doudchenko and Imbens, 2016] read.

[Ferman, 2021] read.

[Frangakis and Rubin, 2002] not read.

[Rosenbaum and Rubin, 1983] not read

[Rubin, 1974] not read.

2.5. Extensions/ Developments

[Abadie and L'Hour, 2021] read.

[Amjad et al., 2018] read.

[Ben-Michael et al., 2021] read.

[Ben-Michael et al., 2021] not read.

[Kellogg et al., 2021] not read.

[Kuosmanen et al., 2021] not read.

[Muhlbach and Nielsen, 2019] read.

Developments

[Arkhangelsky et al., 2021] not read

[Athey et al., 2017] not read.

[Brodersen et al., 2015] read.

[von Brzeski et al., 2015] read.

[Hartford et al., 2017] read.

2.6. Testing

[Andrews, 2003] not read.

[Cattaneo et al., 2021] not read.

[Chernozhukov et al., 2019] not read.

[Chernozhukov et al., 2021] not read.

[Firpo and Possebom, 2018] not read.

[Hahn and Shi, 2017] read.

2.7. Time Series Econometrics

[Martin et al., 2012] read.

[Harvey and Thiele, 2020] read.

[Breitung and Knüppel, 2021] partially read.

3. Theory

In this chapter, we propose alternative SC-estimators to assess the magnitude of treatment effects in observational settings. To establish a general basis, we first describe the contextual environment of the estimation. Similar to the setting as introduced by ADH, we consider a framework with $J+1$ panel units indexed by $j = 0, 1, ..., J$ that are observed over a time horizon of T periods. Without loss of generality, assume that unit $j = 0$ is exposed to the treatment at period $t = T_0$ with $1 < T_0 < T$ and that there are no treatment anticipation and contamination (i.e., no spillovers in time and space). The former would be the case if the treatment affects unit $j = 0$ before T_0, the latter describes the case where some of the supposedly untreated units $j = 1, ..., J$ are contaminated as they are affected by the treatment. To contextualize these assumptions, [Abadie et al., 2010] argue that in the presence of anticipation effects, T_0 could be shifted back in time until the no-anticipation assumption seem plausible. If panel units in the donor pool[1] are affected by the treatment (contamination) as it is likely in the Brexit-application, those units could be removed from the sample prior to the estimation. Our goal is to evaluate the causal effect of the treatment, the specific functional form of which remains unspecified though. This is possible because the main goal of the SC-estimation lies in the precise estimation of the counterfactual. Since the treatment scenario is empirically observable, it is not necessary to specify the specific functional form of the it.

The following chapter is structured as follows: We first describe the canonical estimation procedure as proposed by ADH. Furthermore, ADH propose a model-invariant hypothesis testing approach. As this approach is employed in the further analysis, we also give a brief overview of these principles. Next we build intuition by considering a simple static scenario with only two donor units and one treatment unit. This setting is subsequently generalized to the case with more potential donors. Our extensions diverge from the setting of ADH in two key aspects: First, we remove the weight constraints, leading us to explore regularization as a means to prevent overfitting. Second, we analyze a situation without covariates which drastically reduces the data requirements and causes our algorithm to estimate the counterfactual with a significantly smaller information set.

[1] To ensure direct comparability with the SC literature, we adopt most of the commonly used terms. For example, control group units are labeled as 'donors'.

However, this fact leads us to the necessity of utilizing all available information in an efficient manner and establishes our main contribution: The integration of multivariate time series approaches into the SC-algorithm. The theoretical derivation of this estimator completes the chapter.

3.1. ADH Case

We start by presenting the SC-method and the testing procedure as introduced by ADH. For the sake of comparability and due to its notational clarity, we borrow the employed notation of Abadie and colleagues. In terms of structural design, we build on the thorough presentation of the SC-method and the related hypothesis testing procedure by [Firpo and Possebom, 2018].

Setup

The estimation task can be constituted by the potential outcome framework as introduced by [Neyman, 1923] and elaborated by [Rubin, 1974]. Let $y^I_{j,t}$ be the (potential) outcome for unit j at point t in the presence of the intervention. Likewise, let $y^N_{j,t}$ be the (potential) outcome for j at point t in the absence of the intervention. ADH define the treatment effect of the intervention as

$$\delta_{j,t} = y^I_{j,t} - y^N_{j,t}$$

and introduce the indicator variable $D_{j,t}$ that takes on the value 1 if unit j is treated at period t and the value 0 otherwise. Given the assumed absence of anticipation and contamination, the following outcome is observed

$$y_{j,t} + D_{j,t}\delta_{j,t} = \begin{cases} y^N_{j,t} & (\text{if } j = 0 \text{ and } t < T_0) \text{ or } j \geq 1, \\ y^N_{j,t} + \delta_{j,t} & \text{if } j = 0 \text{ and } t \geq T_0. \end{cases}$$

The goal to estimate the causal treatment effect $(\delta_{0,T_0}, ..., \delta_{0,T})$ therefore boils down to the estimation of the counterfactuals of unit $j = 0$ in the post-treatment phase $(y_{0,T_0}, ..., y_{0,T})$, i.e. on what trajectory would unit $j = 0$ have been, was there no intervention. The basic idea of ADH is to estimate these counterfactuals as a weighted average of the donor outcomes using a data-driven approach to compute the weights. Intuitively, the weights

are computed such that they optimally predict the outcomes and a set of time-invariant explanatory variables for the treatment unit in the pre-intervention phase, conditional on having a percentage interpretation. Thus, for the computation of the weights, we focus exclusively on the pre-intervention time periods $t \in \{1, 2, ..., T_0 - 1\}$. Subsequently, the counterfactuals are extrapolated by applying the calculated weights to the post-intervention time periods $t \in \{T_0, T_0 + 1, ..., T\}$.

Let $Y_j = (y_{j,1}, ..., y_{j,T_0-1})'$ be the vector of observed pre-intervention outcomes for unit j.[2] To distinguish treatment unit and donors, ADH collect the treatment unit in the $((T_0 - 1) \times 1)$-vector Y_0 and row-bind all donor unit vectors into the $((T_0 - 1) \times J)$-matrix Y_1. Moreover, a set of K time-invariant covariates of Y_j is observed for all panel units.[3] Therefore, let X_0 denote the $(K \times 1)$-vector of covariates for Y_0 and let X_1 denote the $(K \times J)$-matrix of explanatory variables for Y_1. To estimate the causal effect of the treatment, the SC-estimator estimates the counterfactuals $(\widehat{y}_{0,1}, ..., \widehat{y}_{0,T_0}, ..., \widehat{y}_{0,T})$ of the single treated unit for the pre- and post-intervation phase as

$$\widehat{y}_{0,t} = \sum_{j=1}^{J} \widehat{w}_j y_{j,t}^N \; \forall \; t \in \{1, ..., T\}$$

The weights $(\widehat{w}_1, ..., \widehat{w}_J)$ are constraint such that $\widehat{w_j} \geq 0 \; \forall \; j$ and $\sum_{j=1}^{J} \widehat{w_j} = 1$. It is worth noting that this constraint requires the counterfactuals to belong to the convex hull of the donors as otherwise, $\widehat{Y}_0$ will never match its true counterpart. [Abadie et al., 2010] argue that "the magnitude of discrepancy" should be calculated in advance of each SC-application. If the researcher finds that the pre-intervention values of Y_0 fall outside the convex hull of the donors, the usage of SC is not recommended. Formally, $(\widehat{w}_1, ..., \widehat{w}_J)$ is the solution of the following nested optimization problem:

$$\widehat{w}(v) = \arg\min_{w} \sum_{k=1}^{K} v_k \left(x_{0,k} - \sum_{j=1}^{J} w_j x_{j,k} \right)^2$$

with v being an arbitrary positive definite vector of dimension $(K \times 1)$ which solve the

[2] For instance, in the canonical example of [Abadie and Gardeazabal, 2003], Y_0 would be the vector of GDPs for Great Britain until the Brexit referendum.

[3] In the already mentioned Brexit-example, natural predictors of GDP are its components consumption, investment, government spending and net exports.

second optimization problem:

$$\widehat{v} = \arg\min_{v} \sum_{t=1}^{T_0-1} \left(y_{0,t} - \sum_{j=1}^{J} \widehat{w}_j(v) y_{j,k} \right)^2$$

Afterwards, the causal effect of the intervention $\delta_{j,t}$ can be quantified at each time point after the intervention $t \in \{T_0, T_0+1, ..., T_1\}$ as the gap between observed $(y_{0,t}^N + \delta_{j,t})$ and predicted outcome $(\widehat{y}_{0,t}^N)$.

This two-step estimation procedure serves two crucial purposes: $\widehat{v}$ measures the relative importance of the K variables in X_1 to explain X_0. In contrast, the weighting vector $\widehat{w}(v)$ quantifies the relative importance of each unit in the donor pool. Summarizing the key concept of ADH, the SC-method ensures that the synthesized treatment unit is as similar as possible to the actual treatment unit with respect to the quantity of interest and a set of potential explanatory variables in the pre-treatment period. Especially in the canonical examples of SC, the quantity of interest (e.g. GDP) and the explanatory variables (e.g. consumption, investment, government spending and net exports) are interconnected by construction. Thus, observing that the SC-estimator was capable of approximating both targets significantly enhanced the methods credibility. If the explanatory variables are omitted, the SC-algorithm reduces to an Ordinary Least Squares (OLS) estimation, constraint to have no constant and weakly positive coefficients that sum up to one.

Hypothesis Testing

The question of treatment effect significance arises naturally subsequent to the construction of the synthetic control. ADH propose a model-invariant non-parametric inference procedure that is based on the Exact Hypothesis Test proposed by [Fisher, 1935]. The basic idea behind such permutation tests is to compare the observed data with a number of randomly permuted versions of it, and to use the distribution of the test statistic calculated of the permuted samples to estimate the probability that the observed result occurred by chance alone.

In the context of SC, ADH consider permutations in region (i.e. panel unit) and time. Region permutations estimate the treatment effect vector $(\delta_{j,T_0}, ..., \delta_{j,T})$ for each panel

unit $j \in \{0, ..., J\}$.[4] This procedure provides the researcher with the empirical $(J+1)$-observational distribution of the treatment. Next, it is possible to compare the estimated treatment vector $(\delta_{0,T_0}, ..., \delta_{0,T})$ of the truly treated unit with the J placebo-treatment vectors of the units of the donor pool. Given the estimated treatment effect for $j = 0$ is large, the null hypothesis of no treatment effect can be rejected at the significance level of one minus the percentile of $(\delta_{0,T_0}, ..., \delta_{0,T})$ in the empirical distribution.[5] Time permutations on the other hand consider only panel unit $j = 0$, permute T_0 to dates prior to the true treatment date and compute again the empirical treatment distribution. Given that $T_0 >> J$, this approach can increase the sensitivity of the test, since the theoretically feasible significance threshold of region permutation tests is determined by $\frac{1}{J}$. For both, region and time permutations, ADH condense the vector of estimated treatment effects into a precision metric like the Mean Squared Forecast Error (MSFE)[6] of the following form:

$$MSFE_j = \frac{\sum_{t=T_0}^{T} \left(\widehat{y}_{j,t}^N - y_{j,t}^N\right)^2}{T - T_0}$$

A possible problem that can occur when assessing the relative rarity of the estimated treatment effect using the procedure described above is the existence of outliers in the donor pool. In the context of region permutations, suppose that a donor region is very different from the rest such that it falls outside the convex hull of the remaining donors. Note, that this circumstance does not cause problems for the truly treated region and its synthesized counterfactual as we expect the SC-algorithm to assign a near zero weight to such an outlier. However, since the outlier itself cannot be synthesized precisely by the donor pool, both MSPE and MSFE are expected to be large. As this special feature causes the permutation test to be unreasonably conservative, ADH propose to exclude regions that are hard to predict, i.e. who have a MSPE that exceeds the MSPE of the truly treated unit to a great extent. Figure 1 visualizes the exclusion procedure in the tobacco control application of ADH.

[4] Note that it is necessary to exclude the truly treated unit from donor pool to ensure the validity of the no contamination assumption.

[5] For instance, let $J = 99$ such that treatment effects for 100 panel units can be computed. As long as the estimated treatment effect of the truly treated units belongs to the 95 largest effects (95th percentile or higher), the permutation test rejects the null hypothesis of no treatment effect at least at 5 percent.

[6] Note that ADH speak of the Mean Squared Prediction Error for dates before and after T_0. Since we consider the time span until T_0 as prediction window and the time span after T_0 as forecast window, we employ the label Mean Squared Prediction Error (MSPE) before T_0 and the label MSFE from T_0 onward.

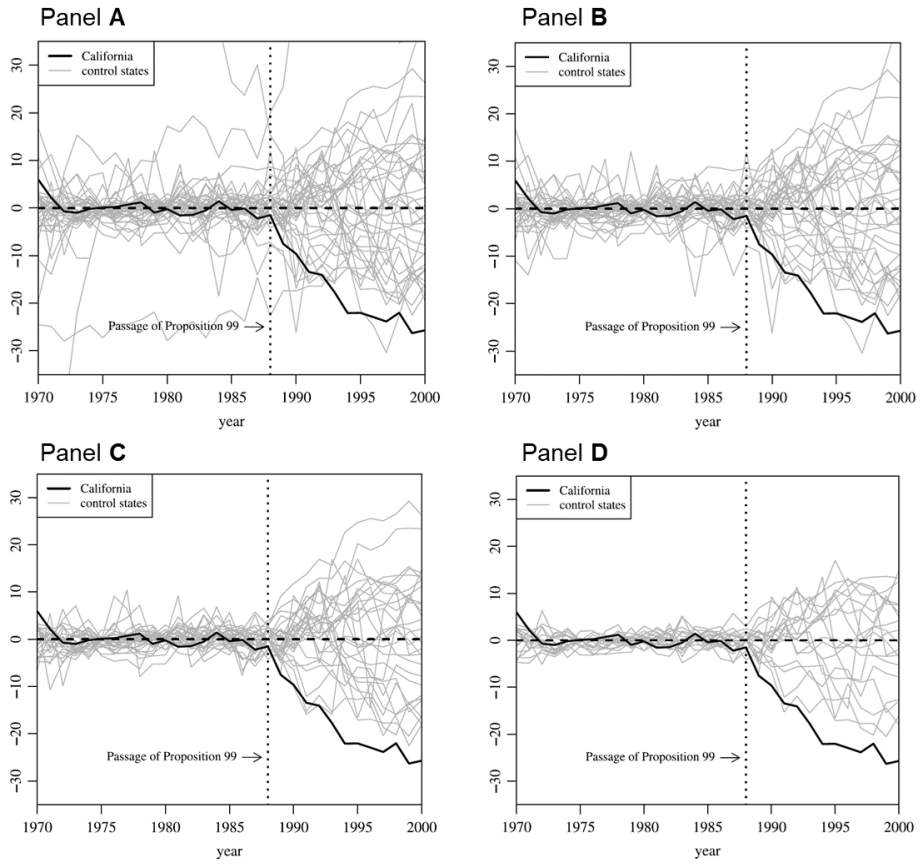

Figure 1. Region Exclusion Procedure of ADH

The vertical axis indicates the gap between observed and estimated per capita cigarette sales, the bold line represents the truly treated region (California). Two observations stand out when considering panel A: First of all, the treatment has a clear negative effect for California. Second, some regions have both a poor pre- and post-treatment fit. Since the treatment significance should not be artificially driven by regions with poor fit, ADH successively remove regions with a large MSPE relative to California. Panel B excludes regions with a MSPE that is more than 20 times as large the MSPE of California, Panel C lowers the cutoff to five times California's MSPE and Panel D to two times the MSPE. In the last scenario, only 19 regions are left and California is the one with the most extreme treatment effect. The authors therefore conclude that the treatment is statistically significant with a (permutation) p-value of 5,3% $\left(\frac{1}{19}\right)$.

One way to bypass the inefficient sample reduction procedure is to look at the distribution of the ratios of MSFE and MSPE. By scaling the post-treatment fit by the

pre-treatment fit, regions with a poor fit are implicitly controlled for. In the tobacco control application, California is the region with the highest MSFE-to-MSPE ratio among all 39 regions which translates into a p-value of 2,6% $\left(\frac{1}{39}\right)$.

3.2. Simple Static Extension

To provide an intuitive introduction for our proposed extensions, we first consider the most simple scenario of one treatment unit $j = 0$ and two donor units $j = 1, 2$. We consider a setting where only the outcome series (e.g. GDP) and no further covariates (e.g. consumption, investment etc.) are observed. It is assumed that before $t = T_0$ the units have a joint distribution of the form[7]

$$Y = \begin{pmatrix} y_0 \\ y_1 \\ y_2 \end{pmatrix} \sim \mathcal{N}(\mu, \Sigma) \text{ for } t < T_0.$$

with $\mu = (\mu_0, \mu_1, \mu_2)'$ and the positive definite covariance matrix

$$\mathbf{\Sigma} = \begin{pmatrix} \sigma_0^2 & \boldsymbol{\sigma_{12}'} \\ \boldsymbol{\sigma_{12}} & \mathbf{\Sigma_2} \end{pmatrix}.$$

σ_0^2 denotes the variance of y_0, $\mathbf{\Sigma_2}$ is a (2×2) covariance matrix of the vector $(y_1, y_2)'$ and $\boldsymbol{\sigma_{12}}$ is a (2×1) vector with elements $cov(y_0, y_1)$ and $cov(y_0, y_2)$.

Disregarding any constraints, we are interested to derive the best unbiased forecast of y_0 given the controls y_1 and y_2 which is obtained as

$$\begin{aligned} \widehat{y}_0^N &= \mu_0 + w_1^{OLS}(y_1 - \mu_1) + w_2^{OLS}(y_2 - \mu_2) \\ &= \mu^* + w_1^{OLS} y_1 + w_2^{OLS} y_2 \end{aligned}$$

where $\mu^* = \mu_0 - w_1^{OLS}\mu_1 - w_2^{OLS}\mu_2$. This forecast can be directly estimated by an unrestricted OLS regression of y_0 on y_1 and y_1. However, the result implies that there is no inherent reason to impose the restrictions that $w_1^{OLS}, w_2^{OLS} \geq 0$ and $w_1^{OLS} + w_2^{OLS} = 1$.

[7] For the ease of exposition we suppress the time index t as in this section we neglect any dynamic effects which will be considered in the next section.

Furthermore, we argue that the construction of SC should include a constant term, as otherwise the estimated counterfactual may have a mean outside the convex hull of the donor means. See also [Doudchenko and Imbens, 2016] for a careful discussion of these restrictions.

For illustrative reasons, assume that

$$Y \sim \mathcal{N}\left(\begin{pmatrix}1\\1\\1\end{pmatrix}, \begin{pmatrix}1 & 0.1 & 0.4\\0.1 & 1 & 0.5\\0.4 & 0.5 & 1\end{pmatrix}\right).$$

For this example the unrestricted optimal weights for the counterfactual result as $w_1^{OLS} = -0.1333$, $w_2^{OLS} = 0.4667$ and $\mu^* = \mu_0 - w_1^{OLS} \cdot \mu_1 - w_2^{OLS} \cdot \mu_2 = 0.6667$.[8] Note that w_1^{OLS} is negative even though all bivariate correlations between the units are positive. One may argue that this result does not make much sense as the economic interpretation of y_1 entering the counterfactual $\widehat{y}_0^N$ with a negative sign is unclear. This demonstrates the trade-off between optimality in a statistical sense and the economic interpretation of the solution.

What happens if we impose the restrictions that all weights are positive and sum up to unity? In this case the restricted optimum yields the linear combination $\widetilde{y}_0^N = 0.2y_1 + 0.8y_2$. The important difference lies in the variance of these estimates. For our example we obtain

$$var(y_0 - \widehat{y}_0^N) = 0.8267$$
$$var(y_0 - \widetilde{y}_0^N) = 1.1600.$$

It is interesting to note that the variance of the restricted estimate is even larger than the unconditional variance of y_0. This is possible as $(w_1, w_2) = (0, 0)$ is not included in the restricted parameter space.

So far we argued and showed illustratively, that an unrestricted OLS estimate can be superior to the constraint SC estimate in settings with few panel units and a clear correlation structure among the units. This indication will be further refined in subsequent

[8] The derivation of the employed estimators is postponed to the appendix.

Monte Carlo simulations. In microeconometric settings it is usually assumed that the units in the treatment group and units in the control group are uncorrelated. In such cases the construction of a SC is unpromising as the dependency between treatment unit and donors is the core condition for a plausible estimation of the counterfactual. If no such relationship exists, the optimal estimate boils down to $\widehat{y}_0^N = \mu_0$ and, therefore, it does not make sense to involve a SC. In macroeconomic applications however, the variables in the treatment and control group (e.g. GDP) are typically correlated and it is therefore important to model this relationship. As the simple scenario with only two panel units in the donor pool is unrealistic in practice, we now move to the general static case with $J+1$ panel units.

3.3. General Static Extension

In empirical macroeconomic practice, the observed time series are typically low- frequency, i. e. the quantities of interest are measured at monthly, quarterly or even annual intervals. Thus, the number of pre-intervention time periods $(T_0 - 1)$ is typically small and may even be smaller than the number of units in the donor pool J. In such scenarios, the unrestricted OLS estimate may face issues of instability or, in the case of $T_0 - 1 < J$, due to singularity, it may not even be identified. So see this, let us now consider the statistical properties of the corresponding least-squares estimator for an arbitrary J:

$$y_{0,t} = \mu^* + w_1 \; y_{1,t} + w_2 \; y_{2,t} + ... + w_J \; y_{J,t} \text{ for } t = 1, 2, ..., T_0.$$

From standard results on least-squares regressions it follows that for fixed J and $T_0 \to \infty$ the OLS estimator $\widehat{w} = (\widehat{w}_1, ..., \widehat{w}_J)$ is unbiased and converges in probability to the Mean Squared Error (MSE) optimal weights w. In empirical practice, we typically have a large number of donors candidates such that J may be of similar magnitude than T_0. In this case, $\frac{J}{T_0}$ is substantially larger than zero and, therefore, some regularization is required. Indeed, as shown in the next proposition, the OLS estimator is inconsistent in such cases:

Proposition 1 *Let* $Y_t = (y_{0,t}^N, y_{1,t}, \ldots, y_{n,t})'$, $\widehat{y}_{0,t}^N = \widehat{w}' x_t$, $x_t = (y_{1,t}, \ldots, y_{n,t})'$ *and* $\widehat{w}$ *denote the OLS estimator of* w *from above. If* y_t *are independent draws from* $Y \sim \mathcal{N}(\mu, \Sigma)$

for $t = 1, \ldots, T_0, \ldots, T$ *then for* $T_0 \to \infty$ *and* $n/T_0 \to c > 0$ *it follows that* $\widehat{Y}^N_{0,t} - Y^N_{0,t}$ *is asymptotically distributed as* $\mathcal{N}(0, c)$ *for* $t > T_0$.

It is important to note that the OLS estimator does not converge if both the number of pre-treatment observations and the number of regressors tend to infinity at the same rate. Similar results were obtained by [Bekker, 1994] who considers the asymptotic distribution of $\widehat{w}$. Our result is simpler as we consider some particular linear combination given by $\widehat{w}' x_t$ where $t > T_0$. In this case the distribution does not depend on the covariance matrix Ω.

The issues of external validity and overfitting are closely related to the aspect of identification. Especially when employing non-parametric statistical learning methods, it is simple to achieve a high in-sample (pre-treatment) fit. The crucial part when dealing with forecasts is that the observed in-sample patterns generalize well outside the verifiable horizon (post-treatment). ADH solve this issue by restricting the weights to be non-negative and to sum up to one. Besides preventing the model from overfitting, the percent restriction guarantees the existence of unique weights, especially when dealing with a small number of pre-treatment periods. Regularized regressions constitute another model family that is capable of balancing the trade-off between under- and overfitting.

ELASTIC NET

In this context [Doudchenko and Imbens, 2016] suggest employing an elastic net regression to regularize the donor weights. It solves the following objective function:

$$Q(w, \lambda_1, \lambda_2) = \sum_{t=1}^{T_0 - 1} \underbrace{\left(y_{0,t} - \mu^* - \sum_{j=1}^{J} w_j y_{j,t} \right)^2}_{RSS} + \lambda_1 \underbrace{\left(\sum_{j=1}^{J} w_j^2 \right)}_{Ridge} + \lambda_2 \underbrace{\left(\sum_{j=1}^{J} |w_j| \right)}_{Lasso}$$

The L_2-norm (Ridge-Penalty) is a continuous shrinkage method, that shrinks the coefficients towards zero without performing variable selection in the sense that certain coefficients are set exactly to zero ([Hoerl and Kennard, 1970]). However, it has the appealing feature that its estimation only involves the addition of a diagonal matrix to the Residual Sum of Squares (RSS). Therefore, the objective function keeps an explicit closed form solution which is particularly important if the sample is small.

In contrast, the L_1-norm (Lasso-Penalty) as proposed by [Tibshirani, 1996] penalizes the sum of the absolute values of the coefficients. The nature of the penalty term causes this regularization to perform both, continuous shrinkage and automatic variable selection. As a consequence, the argmin vector of the objective function typically contains many entries that are exactly zero which makes the resulting model sparse and easier to interpret. However, since the absolute value function is not continuously differentiable, the Lasso has no closed form solution. Consequently, the minimum of the objective function has to be approximated, which is typically done via numerical optimization techniques like cyclical coordinate descent algorithm (see for example [Friedman et al., 2010]). The shrinkage parameters λ_1 and λ_2 can be selected through k-fold Cross Validation (CV). This involves storing combinations of λ_1 and λ_2 that minimize the objective function across k validation sets. The average value of these hyperparameters is then computed to make the final choice.

REGSC

We propose a different regularization that we call the regularized synthetic control estimator. This estimator augments the OLS objective function by a Ridge penalty and a simple "inverse" Ridge that shrinks the coefficient sum towards one. The objective function has the following form:

$$Q(w, \lambda_1, \lambda_2) = \sum_{t=1}^{T_0-1} \underbrace{\left(y_{0,t} - \mu^* - \sum_{j=1}^{J} w_j y_{j,t}\right)^2}_{RSS} + \lambda_1 \underbrace{\left(\sum_{j=1}^{J} w_j^2\right)}_{Ridge} + \lambda_2 \underbrace{\left(1 - \sum_{j=1}^{J} w_j\right)^2}_{\text{"}inverse\text{" } Ridge}$$

Due to the individual shrinkage to zero (Ridge) and the joint shrinkage to one (inverse Ridge), this regularization is closely related to original SC estimator but in contrast to the elastic net, it is flexible enough to produce non-zero weights that are directly interpretable. Moreover, as it does not involve approximating the gradient of the absolute value function, it has the following closed form solution:

$$\widehat{w}_{\lambda_1,\lambda_2} = (Y_1'Y_1 + \lambda_1 I_J + \lambda_2 \mathbf{1_J 1_J'})^{-1}(Y_1'Y_0 + \lambda_2 \mathbf{1_J}).$$

I_J depicts a J-dimensional vector of ones, $\mathbf{1_J 1_J'}$ an all-ones matrix of dimension J. Consis-

tent with the notation of chapter 3.1, Y_1 is a $(T_0-1)\times J$ matrix that stacks all pre-treatment donor observations for $t = 1, ..., T_0 - 1$ and $j = 1, ..., J$. Analogously, Y_0 is a $(T_0 - 1) \times 1$ vector that stacks the pre-treatement time series observations of the treatment unit. To omit the constant from the penalty, a vector of ones should be joined to Y_1 from the left and the first element of I_J respectively $\mathbf{1_J 1'_J}$ be set to zero. Alternatively, Y_0 and Y_1 could be demeaned prior to an intercept-free estimation. In the appendix, we show for $\lambda_1 \to \infty$ and $\lambda_1/\lambda_2 \to c$ the weights converge to $1/(n + c)$, which seems to be a more reasonable target than shrinking towards zero as done by the elastic net. Similar to the case of the elastic net, the shrinkage parameters λ_1 and λ_2 can by chosen by cross validation, where our experience suggests that optimizing subject to the restriction $\lambda_1 \approx 10,000 \cdot \lambda_2$ reduces computation time and already produces reasonable estimates.

The combination of a closed form solution and tuneable hyperparameters make the REGSC-method highly appropriate for the low-frequency macroeconomic context of SC: It is able to produce weights that are interpretable, flexible and efficiently estimated in small samples. These characteristics are empirically verified in the subsequent simulation study. Besides the proposed regularization, we also implemented a numerically solvable combination of the Lasso- and the "inverse"-Ridge-penalty. In large data sets of a least 1,000 observations, this alternative was competitive to the elastic net and the proposed REGSC-estimator. However, as we are looking for an estimator with robust small sample properties, the Lasso-"inverse"-Ridge estimator is omitted from the further analysis.

3.4. General Dynamic Extension

TBD: When modeling macro time series it is often assumed that the $(J+1)\times 1$ vector of time series $y_t = (Y_{0,t}, ..., Y_{J,t})'$ can be represented by a VAR model of the following form:

4. Simulation

In this chapter, we empirically test the performance of our proposed and the already existing estimators for SC in different DGP. Independent of the specific features of the data in terms of pre- and post-treatment period length and the prevailing time series structures, we proceed as follows: We simulate $T_{pre} = T_0 - 1$ periods of pre-treatment and $T_{post} = T - (T_0 - 1)$ periods of post-treatment data for the single treated unit and the J donor units. Each estimator's main goal is to grasp the consistent patterns before treatment and accurately extend these into the time after treatment. Said differently, the pre-treatment phase depicts the training set of the models and the post-treatment the validation set. To root the simulation framework as close as possible to real-world SC applications, we define T_{pre} and T_{post} such that their range is comparable to low-frequency macroeconomic settings, i.e. $T_{pre} \in \{20, 50, 100\}$ and $T_{post} \in \{10, 20, 30\}$. Furthermore, we consider two types of DGP, a static factor process and a dynamic VAR-process that is inspired by real GDP processes of the G20 countries.

4.1. Static Data Generating Processes

4.1.1. Set up

In their SC application of estimating the causal effect of California's proposition 99, [Abadie et al., 2010] suppose that the (potential) outcome $Y_{i,t}^N$ follows a factor model of the form

$$Y_{i,t}^N = \alpha_t + \theta_t Z_i + \lambda_t \mu_i + \epsilon_{it}.$$

α_t denotes an unknown panel-invariant intercept, Z_i is a vector of observed panel-specific covariates, θ_t is a vector of unknown parameters, λ_t is a vector of unknown common factors and μ_i are panel-specific unknown factor loadings. The unobserved shocks ϵ_{it} have zero mean at the panel level. For this specific setting, [Abadie et al., 2010] show that "[...] the bias of the SC-estimator can be bounded by a function that goes to zero as the number of pre-treatment periods increases." Further the number of donor units has to be fixed. The fact, that α_t is panel-invariant seems minor at first glance. However, as the SC-estimator does neither contain an intercept nor does it allow for extrapolation outside the convex hull of the donor pool, the unbiasedness of the estimator directly depends on

the distribution of the intercepts. In a slightly more realistic data-generating scenario, the intercepts do not follow a degenerate point distribution with $P(X = 0) = 1$ but are drawn from a symmetric distribution centered around the origin like the standard normal.

[Ferman, 2021] considers a de-meaned scenario without additional covariates. In our static simulation, we follow the basic set-up of Ferman and generate data according to a similar factor model. However, we consider it more realistic to add a time-invariant and panel-specific intercept to the (potential) outcome instead of analyzing a de-meanded DGP. Our representation of the counterfactuals therefore boils down to

$$Y_{i,t}^{N} = \alpha_i + \lambda_t \mu_i + \epsilon_{it}.$$

In this simplified setting, the counterfactual is given by the composition of the unknown panel-specific factor loadings μ_i and the F unknown common factors $\lambda_t = (\lambda_{1,t}, ..., \lambda_{F,t})$ plus intercept α_i and idiosyncratic shocks ϵ_{it}. For the sake of simplicity, Ferman considers a scenario with only two common factors, $\lambda_{1,t}$ and $\lambda_{2,t}$. We proceed analogously and generate data such that the (potential) outcome of the treated unit and the first half of the donor pool load exclusively with loading one on the first factor, the remaining donors load exclusively with loading one on the second factor. Therefore μ_i is a (2×1)-vector with the first (second) entry being one and the second (first) entry being zero for the first (second) half of the donor pool. Further, the random variables $\alpha_i, \lambda_{1,t}, \lambda_{2,t}$ and ϵ_{it} are realizations of independent and identically distributed (iid) standard normal distributions $\mathcal{N}(0, 1)$. The following figure exemplifies the functionality of the DGP with $T_{pre} = 20$ and $T_{post} = 10$ and a constant treatment effect of $\delta_{0,t} = 10$ for $t > T_0$.[9]

[9] In this example, the series $j = 0$ is treated at $T_0 = 20$, while the impact of the treatment becomes noticeable one time period later, starting from $t > T_0$. Note that the actual treatment effect is irrelevant for our investigation as it is empirically observable.

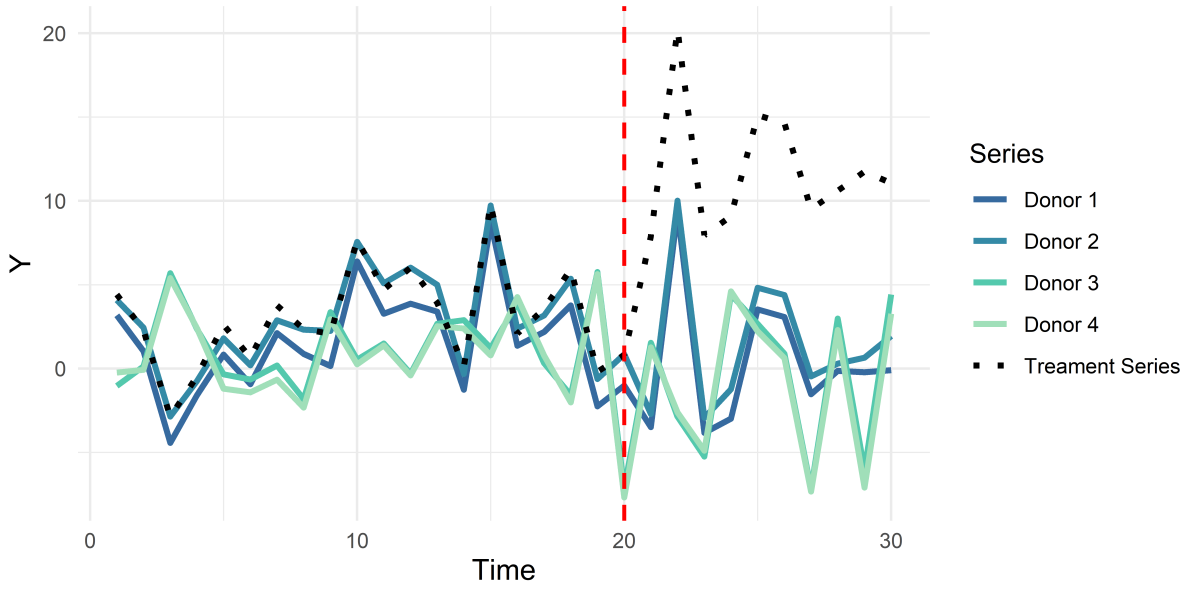

Figure 2. Example Factor-DGP

To make the factor structure more tangible, we scaled the factor variances by 10^1 and the error variance by 10^{-1}. The generated data exhibits a clearly observable factor structure: The treatment unit and the first half of the donors (Donor 1 and 2) as well as the second half of the donors (Donor 3 and 4) share a common factor. Thus, the objective of each employed method is to recover the true factor structure, i. e. irrelevant of the size of donor pool to weight only the first $\frac{J}{2}$ donors positively. Further, we see that each series possesses an own intercept. Yet in this specific example only, the intercept variation is dominated by the factor variation due to the scaling of the variances.

4.1.2. Employed Models

For the static factor DGP, we employ five models:

SC: The first model is the ordinary SC method without additional covariates. Therefore, this method is equivalent to a restricted OLS regression that regresses the treatment series on the donors series given the constraint of no intercept and non-negative coefficients that sum up to one. The belief for this model is that the untuneable restrictions prevent it from overfitting the pre-treatment data but that this inelasticity comes at the costs of a reduced predictive performance.

OLS: The second model is a usual least squares regression that regresses the treatment

series on the donor series. The belief for this model is that it starts overfitting the pre-treatment data as J grows large as it exhibits no regularization opportunities. Further for $J > T_0$, it is unable to provide any prediction or forecast.

NET: The third model we consider is the aforementioned elastic net as proposed by [Doudchenko and Imbens, 2016] in the context of SC. Due to its flexible hyperparameters tuned via pre-treatment CV, we expect this model to perform reasonable well over the entire observation window (pre- and post-treatment). For the sake of simplicity and due to the short training periods, we perform a simple 3-fold CV and rely on highly efficient R-implementation of the elastic net "glmnet" of [Friedman et al., 2010]. In the conceptual introduction of the elastic net, we stressed the potential drawback of having no closed form solution. As a consequence, we assume the model to perform worse in small samples.

REGSC: The fourth model under consideration is our proposed regularized synthetic control model which can be interpreted as a mixture of the elastic net and the original SC estimator. It is comparable to the elastic net insofar that it simply substitutes the Lasso-shrinkage by the inverse-Ridge-shrinkage. This substitution is motivated by the SC-specificity of having percent-like coefficients. Thus, we expect the model to perform well especially in settings that are comparable to the original SC setting like the static factor DGP. Its closed form solution and the increased flexibility caused by the tuneable hyperparameters make us confident that the model performs equally well in small and in large samples as well as in the pre- and the post-treatment period. To reduce computation time, the optimal hyperparameters are obtained by 2-fold CV in a two-step random grid-search procedure. Random hyperparameter grid search has proven to be more efficient than manual grid search both theoretically and empirically. See for instance [Bergstra and Bengio, 2012] for a careful discussion of hyperparameter optimization. Specifically, we start by spaning a large 50×50 grid with λ_1 ranging from 5 to 3,125 and λ_2 ranging from 10 to 10^7 and randomly select 400 out of the 2,500 λ_1-λ_2-combinations.[10] In the first step of the procedure, we identify the optimal hyperparameter combination of the initialized grid by applying 2-fold CV. Based on the result of the first step, we enclose the potential optimum in the second step by sequentially holding the first and the second

[10] In sensitivity checks, we found that wider intervals did not change the location of the potential optimum. However, the optimal range will always be case-specific.

hyperparameter fixed while increasing and decreasing the remaining hyperparameter on a coarser grid. Note that a more efficient CV-procedure could potentially improve the REGSC-method.

FACTOR: The data of this process is generated such that the common factors and the idiosyncratic component are uncorrelated and that the idiosyncratic errors are mutually uncorrelated. Thus, the static factor model which estimates the common factors as linear function of the donors is the most natural candidate model. The Principal Components (PC) estimator is a popular estimator for the factor model. It is employed as follows: In the pre-treatment period, we obtain the predictions by regressing the treatment series on the "latent" factors. As we implemented a two-factor structure, the factors are computed by multiplying the first two eigenvalues of the covariates matrix of the donors with the matrix of the covariates of the donors. The forecasts for the post-treatment period are obtained by multiplying the factor structure of the post-treatment period with the regression coefficients of the pre-treatment regression. As this model directly build upon the DGP, it is our benchmark-model and we expect it to perform best among all candidates.**(@ Jörg Breitung: why no overfitting?)**

For each of the 9 combinations of pre- and post-treatment period length and the 6 investigated donor group sizes, we simulated 1,000 static factor processes as described above.[11] This extensive simulation provides us with a total of 54,000 processes that are analyzed with respect to the following precision and dispersion metrics in the post-treatment period:

- Root Mean Squared Forecast Error (RMSFE): The RMSFE is the central loss function of our analysis. It has the following form:

$$RMSFE(m) = \left(\frac{1}{T - T_0} \sum_{t=T_0}^{T} \left(y_{0,t} - \delta_{0,t} - \widehat{y}_{0,t}(m) \right)^2 \right)^{1/2},$$

  where m present one of the five employed models. Due to its quadratic nature, it is not only a reasonable approximation to realistic loss structures but also mathematically convenient [Diebold, 2017].

[11] Remember that $T_{pre} \in \{20, 50, 100\}$, $T_{post} \in \{10, 20, 30\}$ and $J \in \{5, 10, 15, 20, 25, 30\}$.

- Bias: The RMSFE is unable to distinguish between over- and underestimation as deviations from the true target quantity are squared. The bias is directly related to the $RMSE$ but provides a more detailed measure in terms of error location. It is computed as follows:

$$BIAS_m = \frac{1}{T - T_0} \sum_{t=T_0}^{T} \widehat{y}_m - (y_{0,t} - \delta_{0,t}),$$

such that negative values indicate under- and positive values overestimation. This precision metric is especially important when analyzing intercept-free models as these models will exhibits a bias whenever the treatment intercept falls outside the donor intercepts.

- MZ regression: The MZ regression tests the forecast optimality in a different and more holistic way by regressing the true value on its predicted value in the post-treatment period:

$$y_{0,t} = \beta_0 + \beta_1 \widehat{y}_{0,t} \text{ for } t \geq T - T_0.$$

If the forecast is optimal, we expect to observe that $(\beta_0, \beta_1) = (0, 1)$, an hypothesis that is directly testable by a simple F-test. In comparison to the RMSE and to the bias, this approach superior because we can report average, simulation-overarching quantities without wiping out crucial details like negative and positive biases. In our simulation, we report the share of iterations in which the F-test was unable to reject the joint hypothesis of $(\beta_0, \beta_1) = (0, 1)$ at the conventional significance level of 5%. The closer this share to unity, the closer the specific forecast to the optimal quantity. Due to varying sample sizes, however, those shares are only interpretable at the within-simulation level.

- Variance: To observe the variability of the employed models, we also compute their intra-simulation variances as

$$VAR(m) = \frac{1}{T - T_0} \sum_{t=T_0}^{T} (y_{0,t} - \delta_{0,t} - \widehat{y}_{0,t}(m))^2$$

4.1.3. Results

The full simulation results in table format can be found in the appendix 8.3.1 where we group the tables at the level of the six analyzed donor quantities. Here, we present the central tendencies of the simulation. To do so, consider the following figure that plots the average RMSFE of the models against the size of the donor pool for $T_{pre} = 50$ and $T_{post} \in \{10, 20, 30\}$.[12]

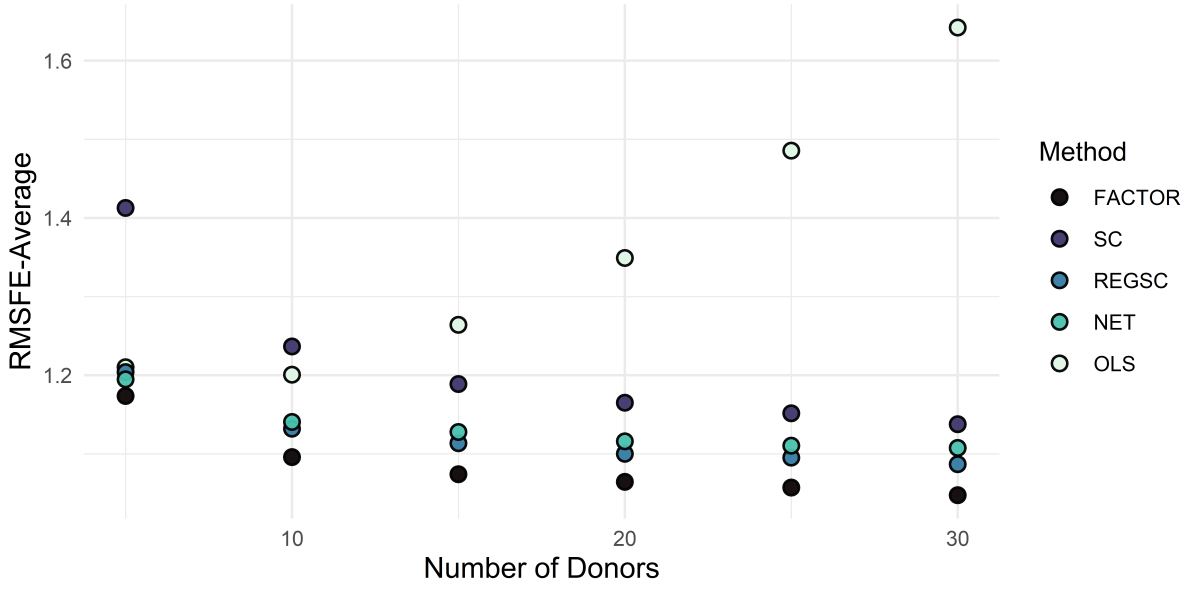

Figure 3. Simulation Performance for $T_{pre} = 50$ and $T_{post} \in \{10, 20, 30\}$

Five observations stand out: First, as expected, the unrestricted OLS regression starts to overfit the pre-treatment data quite fast indicated by a RMSFE that starts to increase from $J = 10$ onward. This tendency consistently aggravates as J approaches T_{pre}. Second, the remaining models seem to successfully distinguish between systematic pre-treatment patterns and noise as their forecasts improve with increasing J. Third, in terms of relative performance, for obvious reasons the factor models extrapolates the process best into the post-treatment period. Fourth, the elastic net and our proposed REGSC estimator perform comparable with slight comparative advantages toward the latter. These differences are more pronounced in small samples which can be interpreted as a sign of the small sample robustness of the closed form solution. Last but not least, the SC estimator,

[12] As we found that the length of the post-treatment period is less important to explain the models performance, we pool the three elements of T_{post} at this point. The depicted means are thus computed on the basis of 3,000 iterations each.

though being able to not overfit the training data, consistently performs worse than the elastic net and the REGSC estimator.

It has been stressed that the RMSFE should not be the only precision metric when evaluating forecast performances as it is unable to detect over- and underestimation. On the other hand, the bias, though being able to flag iteration-specific over- and underestimations, can indicate spurious optimality when it is aggregated.[13] To solve this problem, we can either consider the bias-distributions or rely on the p-values of the MZ-regressions. Let us first focus on the bias-distributions: Above, we argued, that intercept-free models like the SC-method will exhibit a positive (negative) bias whenever the mean of the treatment-series exceeds (falls below) the means of the donor series. In our simulation, the intercepts of the series are iid realizations of standard normal distributions. Therefore, the probability that the mean of the treatment series falls below (exceeds) all donor means equals $\frac{1}{J+1}$.[14] **(@Jörg Breitung: stimmt das? Die Simulationsdaten sprechen dafür)** In the following figure, we extracted the iterations for which the treatment intercept was the most extreme, grouped these observations according to minimum/ maximum and plotted the post-treatment bias density for the SC and the REGSC model.

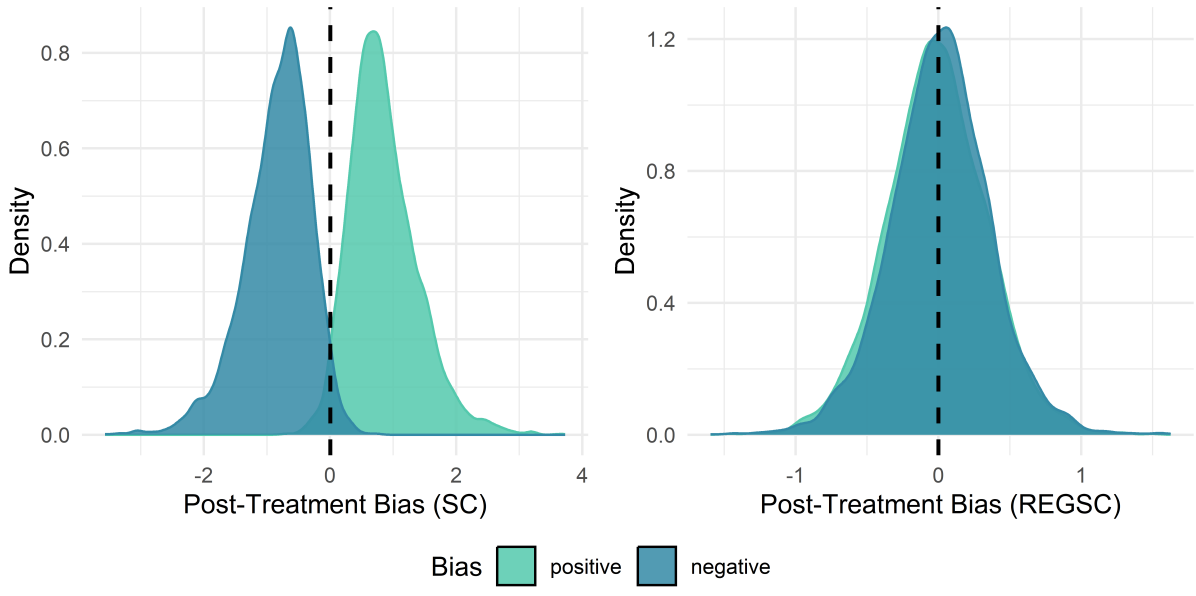

Figure 4. Bias-distribution for the SC and the REGSC model

[13] Consider for instance a model that forecasts $\{-1, 1\}$, each in 50% of the cases for a quantity whose optimal forecast is 0 in 100% of the cases. This model is far from optimal but the mean bias is 0.

[14] For each donor quantity J, there are $(J+1)!$ total orderings. In $\frac{(J+1)!}{J+1}$ of the cases, the intercept of the treatment series is the most extreme. This translate to a probability of $\frac{1}{J+1}$.

The aforementioned problem of the bias becomes immediately apparent: Though both estimator have an average bias that is close to 0, the SC method exhibits severe positive/ negative biases if the treatment intercept falls outside the convex hull of the donors intercepts. In contrast, the REGSC model as well as the remaining models that include an intercepts do not face this issue.

Table S1. Static simulation: Post-treat. share of iterations with MZ-p-value > 0.05

T_0	Donors	SC	OLS	REGSC	NET	FACTOR	2nd best
20	5	0.5650	0.6617	0.7413	0.7648	0.8143	NET
20	10	0.6897	0.3777	0.7577	0.7479	0.8050	REGSC
20	15	0.7343	0.1153	0.7517	0.7486	0.8190	REGSC
20	20	0.7600	NA	0.7380	0.7193	0.7993	SC
20	25	NA	NA	0.7427	0.7420	0.7987	REGSC
20	30	NA	NA	0.7433	0.7475	0.8227	NET
50	5	0.5857	0.8707	0.8677	0.8820	0.8947	NET
50	10	0.7503	0.8237	0.8787	0.8783	0.8940	REGSC
50	15	0.8067	0.7123	0.8703	0.8680	0.8923	REGSC
50	20	0.8103	0.6007	0.8757	0.8717	0.8910	REGSC
50	25	0.8423	0.4333	0.8803	0.8750	0.8920	REGSC
50	30	0.8510	0.3023	0.8680	0.8667	0.8923	REGSC
100	5	0.6200	0.9100	0.9057	0.9150	0.9193	NET
100	10	0.7757	0.9120	0.9207	0.9277	0.9277	NET
100	15	0.8030	0.8743	0.9140	0.9067	0.9183	REGSC
100	20	0.8430	0.8480	0.9180	0.9157	0.9260	REGSC
100	25	0.8540	0.8097	0.9167	0.9117	0.9247	REGSC
100	30	0.8793	0.7537	0.9177	0.9127	0.9257	REGSC

Table S2. Static simulation: RMSFE

T_0	Donors	SC	OLS	REGSC	NET	FACTOR	2nd best
20	5	1.4438	1.3665	1.2988	1.2980	1.2450	NET
20	10	1.2889	1.6212	1.2294	1.2591	1.1620	REGSC
20	15	1.2444	2.4629	1.2133	1.2377	1.1266	REGSC
20	20	1.2144	NA	1.1961	1.2251	1.1132	REGSC
20	25	NA	NA	1.1976	1.2175	1.1081	REGSC
20	30	NA	NA	1.1724	1.2051	1.0901	REGSC
50	5	1.4126	1.2101	1.2038	1.1946	1.1735	NET
50	10	1.2366	1.2006	1.1318	1.1404	1.0957	REGSC
50	15	1.1888	1.2640	1.1134	1.1276	1.0740	REGSC
50	20	1.1649	1.3491	1.0999	1.1157	1.0643	REGSC
50	25	1.1515	1.4856	1.0952	1.1104	1.0570	REGSC
50	30	1.1377	1.6422	1.0867	1.1074	1.0474	REGSC
100	5	1.3886	1.1750	1.1750	1.1689	1.1580	NET
100	10	1.2205	1.1308	1.1044	1.1082	1.0835	REGSC
100	15	1.1695	1.1361	1.0810	1.0879	1.0585	REGSC
100	20	1.1371	1.1575	1.0667	1.0783	1.0443	REGSC
100	25	1.1213	1.1871	1.0580	1.0695	1.0349	REGSC
100	30	1.1105	1.2299	1.0565	1.0704	1.0346	REGSC

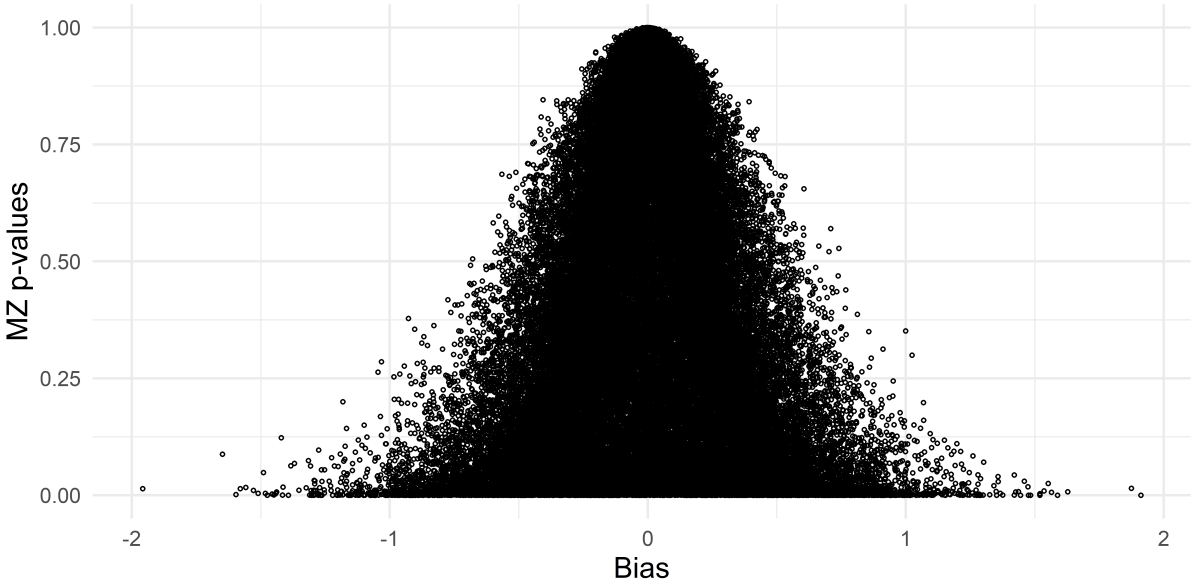

Figure 5. MZ p-values vs. respective bias for the REGSC-model

4.1.4. Summary

4.2. Dynamic Data Generating Processes

5. Simulation

5.1. Static Data Generating Processes

Simulation-Procedure

5.2. Weakly Dynamic Data Generating Processes

5.3. Dynamic Data Generating Processes

In order to rigorously evaluate the performance of both our proposed and existing SC estimators, a key milestone would be to test the estimators in simulated datasets which mimic the real world. As many of the previous studies have focused on the economic development following a treatment, it stands to reason that real world changes in GDP could serve as the basis for a close-to-reality inspired DGP. To ensure that the DGP is based on a relatively uniform reference group which inhibits significant amounts of commonalities and correlation, the data basis consists of all countries from the G20 as well as the European Union (EU). [15] Achtung: R Paket richtig zitieren The dataset is subjected to two additional filters: firstly, only countries with at least 40 years worth of GDP date remain in the dataset, and secondly, the time series selected must be stationary. The latter is tested using the Augmented Dickey Fuller Test. The remaining 22 countries are the base from which the close-to-reality datasets are simulated using a VAR model. Barcelona? Okay!

[15] The GDP data is sourced from the World Bank's World Development Indicators, which is directly accessible through the WDI-Package in R using the ticker 'NY.GDP.PCAP.KD.ZG' (GDP Per Capita Growth Rate).

6. Applications

to verify: elastic net needs to perform time series CV. Not relevant for REGSC as df is split from 1 onward. Also relevant for VAR simulations

We consider three leading examples:

6.1. The Economic Costs of Conflict

[Abadie and Gardeazabal, 2003]

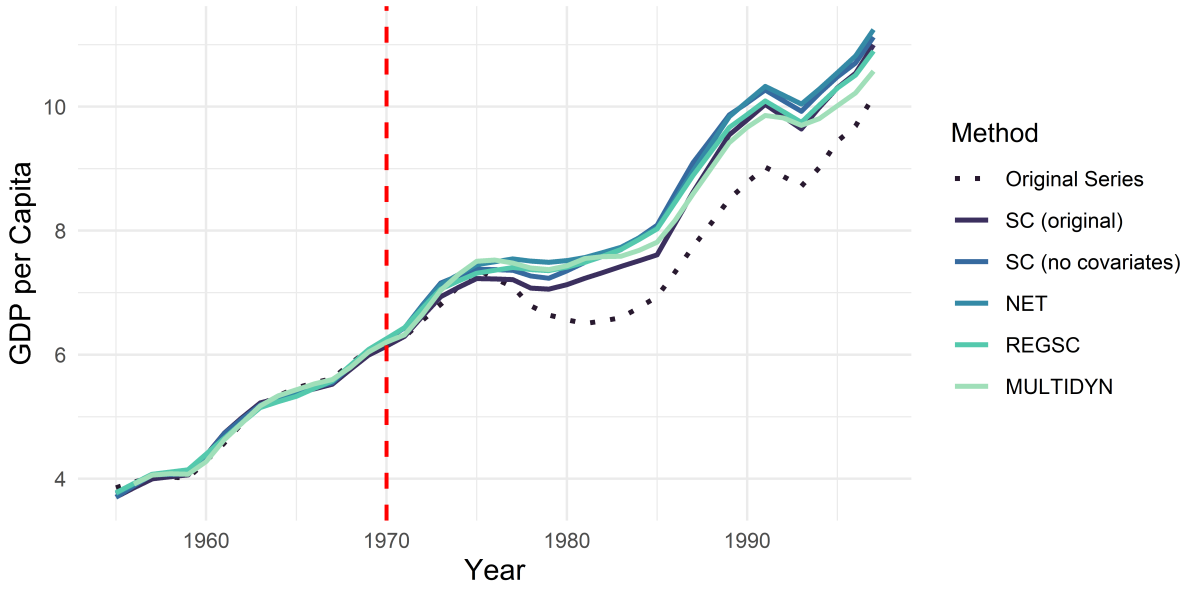

Figure 6. GDP per Capita for the (synthetic) Basque

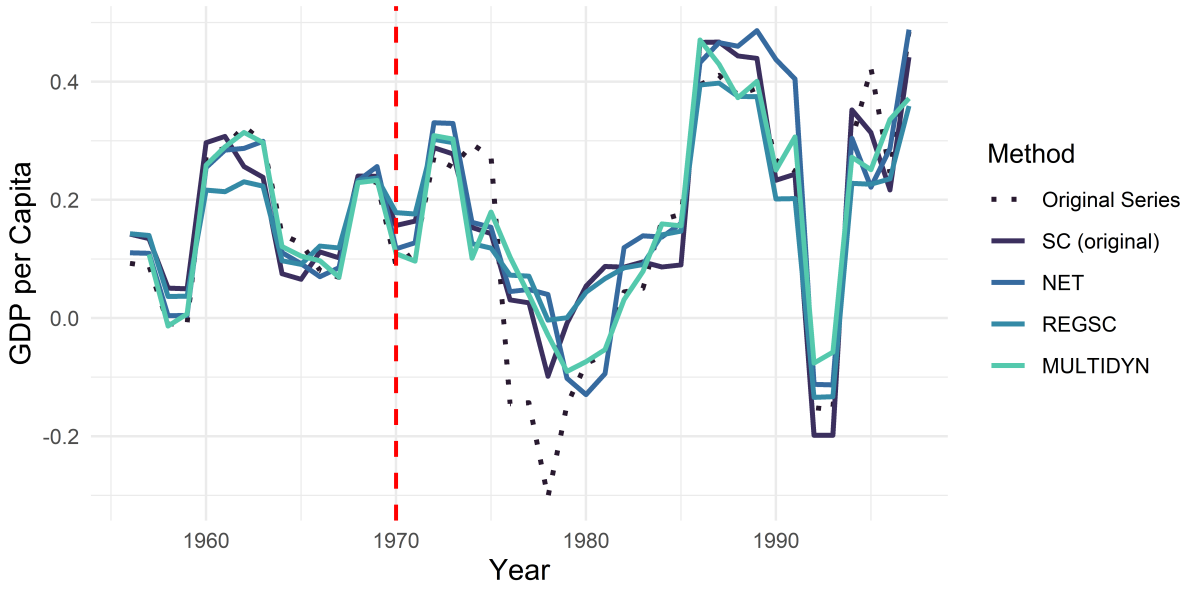

Figure 7. Absolute Growth Rate of GDP per Capita for the (synthetic) Basque

6.2. Estimating the Effect of California's Tobacco Control Program

[Abadie et al., 2010]

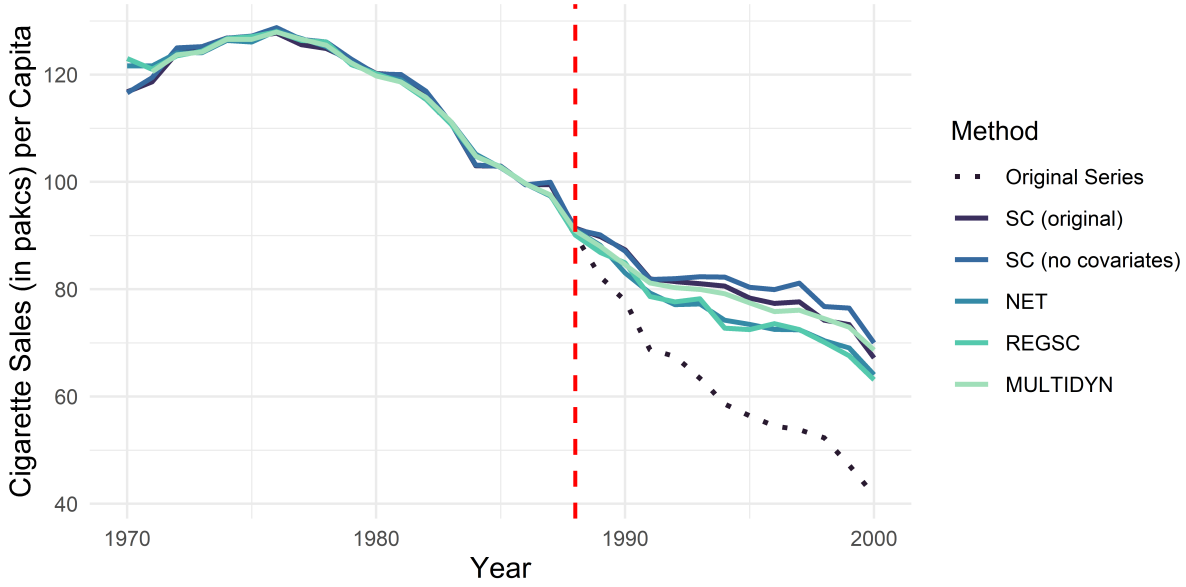

Figure 8. Cigarette Sales per Capita for the (synthetic) California

6.3. The Economic Cost of the 1990 German Reunification

[Abadie et al., 2015]

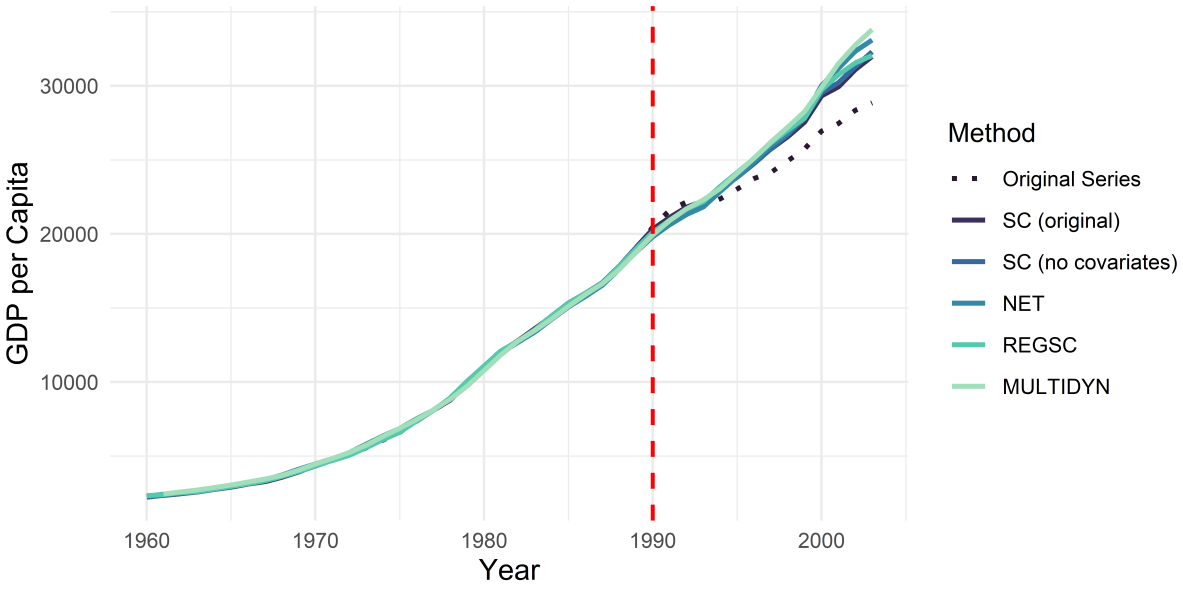

Figure 9. GDP per Capita for the (synthetic) West Germany

7. Conclusion

- Some concluding remarks and an outlook
- Keep short, around 1-2 pages
- Natural extension: case with explanatory variables
- We advocate for an interval instead of an point forecast. Therefore also for an interval estimate of the treatment effect. Report more measures of uncertainty than permutation p-values.

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8. Appendix

8.1. Simple Static Extension

8.1.1. OLS Solution

In case the population covariance matrix is observable, the OLS-coefficients can be directly derived from it: $\left(w_1^{OLS}, w_2^{OLS}\right) = \boldsymbol{\Sigma_2}^{-1}\boldsymbol{\sigma_{12}}$

8.1.2. SC Solution

The restricted solution is can directly be derived from the covariance matrix. The first index in the square brackets indicates the row, the second the column position.

$$\begin{aligned} w_1^{SC} &= \left(\boldsymbol{\sigma_{12}}'[1] - \boldsymbol{\sigma_{12}}'[2] - \boldsymbol{\Sigma_2}[2,1] + \boldsymbol{\Sigma_2}[1,1]\right) / \left(\boldsymbol{\Sigma_2}[1,1] + \boldsymbol{\Sigma_2}[2,2] - 2 * \boldsymbol{\Sigma_2}[1,2]\right) \\ &= (0.1 - 0.4 - 0.5 + 1)/(1 + 1 - 2 * 0.5) = 0.2 \end{aligned}$$

$$\begin{aligned} w_2^{SC} &= \left(\boldsymbol{\sigma_{12}}'[2] - \boldsymbol{\sigma_{12}}'[1] - \boldsymbol{\Sigma_2}[1,2] + \boldsymbol{\Sigma_2}[2,2]\right) / \left(\boldsymbol{\Sigma_2}[2,2] + \boldsymbol{\Sigma_2}[1,1] - 2 * \boldsymbol{\Sigma_2}[2,1]\right) \\ &= (0.4 - 0.1 - 0.5 + 1)/(1 + 1 - 2 * 0.5) = 0.8 \end{aligned}$$

8.1.3. Variances

The variances are derived from the weights and the covariance matrix:

$$\begin{aligned} var(Y_0 - w_1 Y_1 - w_2 Y_2) = var(Y_0) + w_1^2 \cdot var(Y_1) + w_2^2 \cdot var(Y_2) - \\ 2 \cdot w_1 \cdot cov(Y_0, Y_1) - 2 \cdot w_2 \cdot cov(Y_0, Y_2) + \\ 2 \cdot w_1 w_2 \cdot cov(Y_1, Y_2) \end{aligned}$$

8.2. General Static Extension

8.2.1. REGSC: The limit for $\lambda_1 \to \infty$ and $\lambda_2 \to \infty$

For $\lambda_1 \to \infty$ and $\lambda_2 \to \infty$ the objective function reduce to

$$Q(\lambda_1, \lambda 2) = \lambda_1 w'w + \lambda_2 (1 - \mathbf{1}'w)^2$$

The derivative is obtained as

$$\frac{\partial Q(\lambda_1, \lambda_2)}{\partial w} = 2\lambda_1 w + 2\lambda_2(\mathbf{1} - \mathbf{1}\mathbf{1}'w)$$

By setting the derivative to zero and multiplying with $\mathbf{1}$ we obtain:

$$\lambda_1 \mathbf{1}'w + \lambda_2(n - \mathbf{1}'w) = 0$$

where $\mathbf{1}'w = \sum w_i$. Solving for $\mathbf{1}'w$ we obtain

$$\mathbf{1}'w = \frac{1}{1 + \lambda_1/\lambda_2}$$

and due to the symmetry of the objective function with respect to the elements of the weight vector we have

$$w_i = 1/(n + n\lambda_1/\lambda_2)$$

8.3. Simulation Study

8.3.1. Static Simulation results

Table S3. Simulation Results of the Static Factor Model with **J = 5** Donors.

T_0	T_1	**FACTOR**	**SC**	**REGSC**	**NET**	**OLS**
		RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}
20	30	1.2497 (-0.0045) [0.7250] {0.6523}	1.4455 (-0.0086) [0.4210] {1.1179}	1.3064 (-0.0144) [0.6240] {0.6898}	1.3086 (-0.0073) [0.6691] {0.6581}	1.3814 (-0.0098) [0.5140] {1.1678}
	20	1.2509 (0.0137) [0.8240] {0.6143}	1.4575 (0.0331) [0.5360] {1.0690}	1.3054 (0.0063) [0.7460] {0.6619}	1.3048 (0.0085) [0.7516] {0.6137}	1.3661 (0.0114) [0.6650] {1.1028}
	10	1.2344 (-0.0092) [0.8940] {0.6137}	1.4283 (-0.0120) [0.7380] {1.0366}	1.2847 (-0.0075) [0.8540] {0.6505}	1.2804 (-0.0098) [0.8723] {0.6086}	1.3520 (-0.0126) [0.8060] {1.0948}
50	30	1.1831 (0.0147) [0.8510] {0.6459}	1.4185 (-0.0223) [0.4600] {1.0600}	1.2131 (0.0137) [0.8210] {0.6440}	1.2045 (0.0117) [0.8388] {0.5834}	1.2190 (0.0106) [0.8250] {0.7974}
	20	1.1740 (-0.0195) [0.9030] {0.6426}	1.4160 (-0.0106) [0.5630] {1.0680}	1.2044 (-0.0204) [0.8650] {0.6311}	1.1948 (-0.0191) [0.8810] {0.5752}	1.2105 (-0.0166) [0.8750] {0.7879}
	10	1.1635 (-0.0081) [0.9300] {0.5963}	1.4033 (-0.0211) [0.7340] {1.0002}	1.1940 (-0.0115) [0.9170] {0.5850}	1.1845 (-0.0081) [0.9260] {0.5407}	1.2009 (-0.0074) [0.9120] {0.7415}
100	30	1.1645 (0.0047) [0.8920] {0.6494}	1.3955 (0.0381) [0.4950] {1.0668}	1.1811 (0.0053) [0.8700] {0.6405}	1.1733 (0.0048) [0.8930] {0.5931}	1.1799 (0.0052) [0.8870] {0.7238}
	20	1.1576 (0.0026) [0.9220] {0.6389}	1.3796 (-0.0384) [0.6080] {1.0337}	1.1756 (0.0040) [0.9100] {0.6226}	1.1691 (0.0021) [0.9120] {0.5871}	1.1755 (0.0012) [0.9100] {0.7090}
	10	1.1518 (0.0108) [0.9440] {0.5971}	1.3907 (0.0592) [0.7570] {0.9826}	1.1682 (0.0156) [0.9370] {0.5740}	1.1643 (0.0146) [0.9400] {0.5432}	1.1697 (0.0135) [0.9330] {0.6623}

Table S4. Simulation Results of the Static Factor Model with **J = 10** Donors.

T_0	T_1	**FACTOR**	**SC**	**REGSC**	**NET**	**OLS**
		RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}
20	30	1.1575 (-0.0071) [0.7090] {0.7830}	1.2944 (0.0002) [0.5720] {1.0468}	1.2229 (-0.0047) [0.6590] {0.7805}	1.2521 (-0.0093) [0.6416] {0.7714}	1.6286 (0.0018) [0.1790] {2.3061}
	20	1.1611 (0.0060) [0.8220] {0.7459}	1.2759 (0.0197) [0.6920] {1.0523}	1.2336 (0.0071) [0.7590] {0.7777}	1.2591 (0.0036) [0.7495] {0.7613}	1.6075 (0.0045) [0.3520] {2.1785}
	10	1.1675 (0.0040) [0.8840] {0.6883}	1.2963 (0.0127) [0.8050] {0.9859}	1.2318 (0.0004) [0.8550] {0.7180}	1.2659 (0.0006) [0.8513] {0.6984}	1.6276 (0.0181) [0.6020] {2.0705}
50	30	1.1048 (0.0049) [0.8640] {0.7755}	1.2511 (0.0020) [0.6680] {1.0148}	1.1444 (0.0079) [0.8240] {0.7207}	1.1524 (0.0065) [0.8310] {0.6795}	1.2140 (0.0087) [0.7420] {1.1008}
	20	1.0985 (0.0047) [0.8870] {0.7744}	1.2416 (0.0037) [0.7270] {0.9962}	1.1327 (0.0029) [0.8750] {0.7333}	1.1417 (0.0042) [0.8750] {0.6573}	1.2022 (0.0028) [0.8190] {1.0658}
	10	1.0838 (0.0001) [0.9310] {0.7704}	1.2171 (0.0033) [0.8560] {0.9989}	1.1183 (-0.0039) [0.9370] {0.7199}	1.1270 (-0.0034) [0.9290] {0.6649}	1.1857 (-0.0049) [0.9100] {1.0604}
100	30	1.0920 (0.0040) [0.9070] {0.8025}	1.2323 (0.0119) [0.6930] {1.0112}	1.1141 (0.0050) [0.9040] {0.7620}	1.1171 (0.0039) [0.9080] {0.6968}	1.1405 (0.0025) [0.8780] {0.9363}
	20	1.0842 (-0.0057) [0.9360] {0.7858}	1.2232 (-0.0031) [0.7680] {1.0095}	1.1073 (-0.0049) [0.9240] {0.7427}	1.1088 (-0.0064) [0.9280] {0.6902}	1.1333 (-0.0081) [0.9150] {0.9314}
	10	1.0742 (0.0080) [0.9400] {0.7377}	1.2059 (-0.0206) [0.8660] {0.9476}	1.0919 (0.0021) [0.9340] {0.6925}	1.0986 (0.0016) [0.9470] {0.6526}	1.1186 (0.0006) [0.9430] {0.8653}

Table S5. Simulation Results of the Static Factor Model with **J = 15** Donors.

T_0	T_1	**FACTOR**	**SC**	**REGSC**	**NET**	**OLS**
		RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}
20	30	1.1398 (0.0243) [0.7340] {0.7617}	1.2604 (0.0374) [0.6240] {1.0341}	1.2276 (0.0244) [0.6450] {0.8403}	1.2533 (0.0197) [0.6408] {0.7997}	2.4777 (0.0312) [0.0320] {6.3624}
	20	1.1287 (0.0042) [0.8200] {0.7481}	1.2417 (0.0258) [0.7250] {0.9966}	1.2149 (0.0129) [0.7460] {0.8261}	1.2432 (0.0165) [0.7386] {0.7667}	2.4716 (-0.0097) [0.0690] {6.3819}
	10	1.1112 (0.0206) [0.9030] {0.7130}	1.2309 (-0.0240) [0.8540] {0.9484}	1.1973 (0.0224) [0.8640] {0.7702}	1.2167 (0.0247) [0.8660] {0.7234}	2.4393 (0.0245) [0.2450] {6.0769}
50	30	1.0818 (-0.0032) [0.8580] {0.8282}	1.1935 (0.0006) [0.7490] {1.0035}	1.1198 (-0.0019) [0.8230] {0.7778}	1.1356 (-0.0019) [0.8070] {0.7102}	1.2747 (-0.0040) [0.5980] {1.3448}
	20	1.0879 (-0.0133) [0.8950] {0.7705}	1.1975 (-0.0183) [0.8060] {0.9465}	1.1307 (-0.0205) [0.8710] {0.7340}	1.1470 (-0.0188) [0.8760] {0.6722}	1.2892 (-0.0210) [0.6930] {1.2960}
	10	1.0522 (-0.0132) [0.9240] {0.7913}	1.1754 (-0.0098) [0.8650] {0.9519}	1.0896 (-0.0130) [0.9170] {0.7423}	1.1004 (-0.0122) [0.9210] {0.6776}	1.2281 (-0.0011) [0.8460] {1.2605}
100	30	1.0678 (0.0007) [0.8950] {0.8395}	1.1818 (-0.0212) [0.7310] {0.9981}	1.0890 (0.0007) [0.8900] {0.7767}	1.0974 (-0.0004) [0.8730] {0.7073}	1.1415 (-0.0034) [0.8310] {1.0481}
	20	1.0667 (0.0048) [0.9290] {0.8321}	1.1809 (-0.0018) [0.8040] {0.9770}	1.0886 (0.0073) [0.9230] {0.7671}	1.0947 (0.0064) [0.9180] {0.6998}	1.1468 (0.0066) [0.8800] {1.0384}
	10	1.0411 (-0.0059) [0.9310] {0.7897}	1.1459 (-0.0049) [0.8740] {0.9161}	1.0654 (-0.0045) [0.9290] {0.7422}	1.0717 (-0.0057) [0.9290] {0.6802}	1.1200 (-0.0056) [0.9120] {0.9948}

Table S6. Simulation Results of the Static Factor Model with **J = 20** Donors.

T_0	T_1	**FACTOR**	**SC**	**REGSC**	**NET**	**OLS**
		RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}
20	30	1.1228 (0.0074) [0.7090] {0.8229}	1.2229 (-0.0229) [0.6550] {1.0024}	1.2040 (0.0044) [0.6410] {0.8544}	1.2331 (-0.0010) [0.6079] {0.8238}	NA (NA) [NA] {NA}
	20	1.1230 (0.0009) [0.8030] {0.7731}	1.2276 (-0.0067) [0.7520] {0.9833}	1.2093 (-0.0008) [0.7250] {0.8563}	1.2385 (-0.0041) [0.7144] {0.8207}	NA (NA) [NA] {NA}
	10	1.0939 (0.0023) [0.8860] {0.7695}	1.1927 (-0.0226) [0.8730] {0.9365}	1.1749 (0.0033) [0.8480] {0.8217}	1.2036 (0.0031) [0.8357] {0.7697}	NA (NA) [NA] {NA}
50	30	1.0662 (-0.0121) [0.8640] {0.8580}	1.1629 (-0.0148) [0.7290] {0.9743}	1.1007 (-0.0141) [0.8330] {0.7912}	1.1180 (-0.0182) [0.8230] {0.7118}	1.3564 (-0.0184) [0.4080] {1.6096}
	20	1.0702 (0.0004) [0.8810] {0.8213}	1.1693 (0.0055) [0.8070] {0.9484}	1.1085 (-0.0004) [0.8710] {0.7632}	1.1205 (0.0008) [0.8690] {0.7049}	1.3538 (-0.0073) [0.5880] {1.5678}
	10	1.0566 (0.0204) [0.9280] {0.7780}	1.1627 (0.0143) [0.8950] {0.9080}	1.0907 (0.0234) [0.9230] {0.7208}	1.1086 (0.0242) [0.9230] {0.6595}	1.3371 (0.0204) [0.8060] {1.4765}
100	30	1.0512 (-0.0083) [0.9040] {0.8556}	1.1442 (0.0027) [0.7800] {0.9709}	1.0735 (-0.0072) [0.8820] {0.7933}	1.0856 (-0.0073) [0.8890] {0.7236}	1.1639 (-0.0066) [0.7880] {1.1355}
	20	1.0438 (-0.0035) [0.9230] {0.8657}	1.1434 (0.0063) [0.8380] {0.9643}	1.0652 (-0.0009) [0.9260] {0.7987}	1.0763 (-0.0030) [0.9200] {0.7379}	1.1584 (-0.0035) [0.8400] {1.1555}
	10	1.0381 (-0.0014) [0.9510] {0.8044}	1.1236 (-0.0054) [0.9110] {0.8937}	1.0615 (0.0005) [0.9460] {0.7387}	1.0729 (-0.0024) [0.9380] {0.6780}	1.1502 (-0.0035) [0.9160] {1.0692}

Table S7. Simulation Results of the Static Factor Model with **J = 25** Donors.

T_0	T_1	**FACTOR**	**SC**	**REGSC**	**NET**	**OLS**
		RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}
20	30	1.1154 (0.0019) [0.6980] {0.8012}	NA (NA) [NA] {NA}	1.2097 (-0.0002) [0.6280] {0.8593}	1.2225 (-0.0036) [0.6440] {0.7918}	NA (NA) [NA] {NA}
	20	1.1038 (0.0081) [0.8150] {0.7882}	NA (NA) [NA] {NA}	1.2001 (0.0127) [0.7410] {0.8314}	1.2240 (0.0125) [0.7310] {0.7967}	NA (NA) [NA] {NA}
	10	1.1051 (0.0056) [0.8830] {0.7503}	NA (NA) [NA] {NA}	1.1829 (-0.0046) [0.8590] {0.7897}	1.2061 (-0.0038) [0.8513] {0.7621}	NA (NA) [NA] {NA}
50	30	1.0657 (0.0104) [0.8510] {0.8576}	1.1525 (-0.0014) [0.7880] {0.9696}	1.1039 (0.0121) [0.8390] {0.8090}	1.1198 (0.0119) [0.8270] {0.7223}	1.4937 (0.0138) [0.2260] {2.0239}
	20	1.0541 (0.0175) [0.9000] {0.8634}	1.1514 (0.0171) [0.8460] {0.9776}	1.0905 (0.0202) [0.8840] {0.8007}	1.1090 (0.0204) [0.8720] {0.7497}	1.4816 (0.0229) [0.3860] {2.0191}
	10	1.0513 (-0.0003) [0.9250] {0.8091}	1.1507 (0.0100) [0.8930] {0.9253}	1.0911 (-0.0041) [0.9180] {0.7763}	1.1025 (-0.0056) [0.9260] {0.7072}	1.4816 (-0.0103) [0.6880] {1.9764}
100	30	1.0377 (-0.0036) [0.9190] {0.8703}	1.1262 (0.0009) [0.8120] {0.9562}	1.0607 (-0.0028) [0.9070] {0.7995}	1.0719 (-0.0026) [0.9000] {0.7252}	1.1913 (0.0002) [0.7200] {1.2385}
	20	1.0431 (-0.0088) [0.9220] {0.8747}	1.1258 (-0.0108) [0.8450] {0.9594}	1.0656 (-0.0075) [0.9180] {0.8108}	1.0778 (-0.0084) [0.9110] {0.7317}	1.1897 (-0.0028) [0.8160] {1.2371}
	10	1.0240 (-0.0011) [0.9330] {0.8335}	1.1119 (0.0133) [0.9050] {0.9086}	1.0478 (0.0015) [0.9250] {0.7689}	1.0588 (0.0023) [0.9240] {0.6957}	1.1803 (0.0046) [0.8930] {1.1779}

Table S8. Simulation Results of the Static Factor Model with **J = 30** Donors.

T_0	T_1	**FACTOR**	**SC**	**REGSC**	**NET**	**OLS**
		RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}	RMSE (BIAS) [MZ-RATES] {VARIANCE}
20	30	1.0985 (0.0061) [0.7480] {0.8283}	NA (NA) [NA] {NA}	1.1867 (0.0131) [0.6260] {0.8679}	1.2139 (0.0104) [0.6397] {0.8722}	NA (NA) [NA] {NA}
	20	1.0915 (0.0044) [0.8140] {0.8092}	NA (NA) [NA] {NA}	1.1714 (-0.0013) [0.7480] {0.8365}	1.2129 (0.0022) [0.7409] {0.8370}	NA (NA) [NA] {NA}
	10	1.0803 (0.0225) [0.9060] {0.7730}	NA (NA) [NA] {NA}	1.1591 (0.0147) [0.8560] {0.8143}	1.1885 (0.0210) [0.8616] {0.7921}	NA (NA) [NA] {NA}
50	30	1.0552 (0.0048) [0.8600] {0.8536}	1.1492 (0.0185) [0.8060] {0.9671}	1.0989 (0.0050) [0.8220] {0.7988}	1.1206 (0.0012) [0.8270] {0.7556}	1.6600 (0.0062) [0.0960] {2.5759}
	20	1.0488 (0.0021) [0.8990] {0.8787}	1.1312 (0.0090) [0.8560] {0.9519}	1.0819 (-0.0005) [0.8680] {0.8199}	1.1032 (-0.0001) [0.8630] {0.7509}	1.6398 (-0.0045) [0.2330] {2.5723}
	10	1.0381 (0.0031) [0.9180] {0.8092}	1.1327 (-0.0015) [0.8910] {0.8922}	1.0792 (-0.0019) [0.9140] {0.7594}	1.0985 (-0.0064) [0.9100] {0.6948}	1.6269 (-0.0201) [0.5780] {2.3713}
100	30	1.0460 (0.0116) [0.9060] {0.8951}	1.1253 (0.0154) [0.8500] {0.9630}	1.0709 (0.0107) [0.9020] {0.8098}	1.0840 (0.0118) [0.8890] {0.7518}	1.2419 (0.0059) [0.6290] {1.3709}
	20	1.0278 (-0.0039) [0.9380] {0.9037}	1.1067 (-0.0003) [0.8750] {0.9668}	1.0477 (-0.0035) [0.9270] {0.8308}	1.0626 (-0.0021) [0.9210] {0.7497}	1.2180 (0.0002) [0.7660] {1.3736}
	10	1.0300 (-0.0309) [0.9330] {0.8168}	1.0995 (-0.0335) [0.9130] {0.8818}	1.0509 (-0.0339) [0.9240] {0.7504}	1.0646 (-0.0330) [0.9280] {0.6825}	1.2299 (-0.0419) [0.8660] {1.2708}